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Robert Perry

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Robert Perry*

"R&D style" is the accepted designator for the policies, procedures, and preferences that characterize research and development programs. There is, in concept, an American style and a Soviet style. American military R&D is often alleged to be wasteful and ill managed, and marred by cost overruns, performance shortfalls, and schedule slippages. Depending on the preconceptions of the viewer, Soviet military R&D may be characterized as sound and productive, or hidebound and lackluster, or imaginative and fruitful. Most observers agree that it is well funded and abundantly staffed, but that its quality is less certain.

Such images largely derive from American perceptions of the post-Sputnik Soviet-American R&D competition, although some

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*I am indebted to Giles Smith, Michael Rich, Mark Lorell, and Alvin Harman of Rand for suggestions that influenced this paper. I may also have made off with some of their words, having annexed their research. But the views expressed here are ultimately my own. In an abbreviated version, this paper was prepared for and orally presented at a March 1979 conference on military R&D sponsored by Cornell University and the Rockefeller Foundation, as part of a comparison of Soviet and U.S. R&D styles. As noted elsewhere, David Holloway prepared a companion piece on the Soviet Union. The research summarized here was supported by The Rand Corporation, in the public interest, and draws on findings earlier reported to various clients. None of those institutions necessarily subscribe to my conclusions or observations.

observations stem from still more ancient criticisms of American military R&D.*

The image and the reality have little in common. In both countries civil R&D differs from military R&D in so many respects that to each must be attributed its own special features. Within the American defense establishment, the Army, Navy, and Air Force favor different methods of conducting and managing R&D.† The Soviet Air Force, Navy, armored forces, strategic missile forces, and military space programs display a somewhat narrower but still diverse set of style preferences. Within the U.S. Air Force, aircraft, spacecraft, and large electronic systems are developed in ways that differ substantially, as do the several institutions directly responsible for them. At what many consider the level of the least common denominator--the industrial firms that contract for military R&D--differences may be less obvious but no less pronounced: the fashion in which the Lockheed California Company's advanced development organization conducts military R&D projects is as distinctive as the name it carries--"The Skunk Works"--and as formidable of reputation. But the Skunk Works style is readily distinguishable from that of the Lockheed Sunnyvale

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*The literature on American and Russian R&D styles is abundant and of varied quality. A well known study that epitomizes informed opinion of the late 1960s is Arthur J. Alexander, *R&D in Soviet Aviation* (The Rand Corporation, R-589, November 1970). The standard reference on American military R&D at mid-century is M. J. Peck and F. J. Scherer, *The Weapons Acquisition Process: An Economic Analysis* (Harvard, 1962). One of the several attempts to examine the two institutions as they were in the late 1960s is Robert Perry, *Comparisons of Soviet and U.S. Technology*, (The Rand Corporation, R-827, June 1973).

†The widely used and somewhat misleading generalizations for the three service "styles" are (respectively), "arsenal," "bureau," and "contract" R&D.

organization (which develops military spacecraft), and neither resembles that of the Northrop fighter aircraft design group in Hawthorne, California some 20 miles south, or the nearby McDonnell-Douglas group that is responsible for civil transport programs.

Perhaps most important, R&D styles change with time, and not always for the worse.

Here, then, are themes to be developed and questions to be explored. Is there, or has there historically been, a typical American style of R&D? If so, is it changing, and how? Is there a typical Soviet style of R&D to which it can be compared? What are the differences, and how or why are they important? And, at the end, does one or the other have an advantage?

What has earlier been said supports the premise that if there is a distinctive American style of military R&D, it lacks definition and varies from place to place as well as from time to time. Conceivably, the "style" of a group immediately charged with responsibility for an R&D program could influence R&D outcomes more than the formal usages of the sponsoring service or department; thus any generalization about U.S. military R&D should take account of the extreme diversity of R&D approaches encompassed in the many sets and subsets of American R&D programs and projects. Style characterizations are therefore likely to apply only to small lots of similar programs.

Traditionally, many of the "successful" and "unsuccessful" outcomes of various R&D undertakings are attributed to differences of R&D style. Indeed, having attempted to evaluate the influences of

many other variables on R&D program outcomes, expounders on the theory and practice of R&D are frequently driven to accept "style" as a best explanator of success or failure.* Nothing else seems to serve as well.

The principal style elements of successful military R&D, whenever conducted, and by whomever, probably can be epitomized in three broad propositions. First and most important, the management of an R&D enterprise must be responsive to the contemporary state and nature of whatever technology is being manipulated. One discriminant is whether "large" advances in system performance can be extracted from a particular R&D program. Will the state of the technology being exploited support an attempt to leap grandly ahead, or should ambition be limited to smaller and presumably more realistic advances?† In the end, it would seem that successful new weapons more often derive from proven technology than from efforts to shape, push, or contrive immature technology--however well intentioned, well funded, or well managed those efforts may be. History says that technology shapes requirements far more often than requirements effectively pull technology. It follows, then, that the *rate* at

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*The Polaris ballistic missile and the Navaho cruise missile programs of roughly the same era are useful examples.

†For illustrative purposes: the development of the first supersonic bomber may represent a "large" advance; the development of a 10-percent "better" inertial guidance system for a ballistic missile may represent a "small" advance. Which is not to say that one will be intrinsically more likely to succeed than the other. If the technology essential to a supersonic bomber is at hand, the program has a large potential for success; if a 10-percent improvement in guidance accuracy requires a level of precision in gyroscope stability that cannot be achieved, that project may well fail.

which a given technology is advancing may determine the outcome of an R&D enterprise--but that rate of change is little affected by such factors as pulsations in budget levels or program priorities, the skill and dedication of program managers, or the forceful intercessions of a service secretary.

Second, the fundamental goal of R&D is to reduce uncertainty, but uncertainty cannot always be diminished fast enough to ensure program "success." Therefore cancellation must be viewed as one acceptable outcome of *any* R&D project, sometimes vastly preferable to a calculated continuing effort to achieve the unachievable. In some instances remarkable technical accomplishments result from determined assaults on uncertainty, but (as in the recent cases of the Anglo-French and Russian supersonic transports) there may be some lingering doubt that the game was worth the candle.

Third, an unqualified commitment to some monolithic concept, approach, or means of performing some desired function can inculcate a costly failure or a yet more costly "success": the late delivery of expensive military equipment which proves to be of little or no military worth. The several German "V-weapons" of 1944-1945 are striking examples, although some more recent cases can be found.

Some of the perceived differences between Soviet and U.S. R&D approaches derive from culture, tradition, and dogma. For example: it is a Soviet credo that weaponry (which is to say, technology) cannot drive military strategy, but rather that doctrine determines requirements which in turn dictate technology choices. It is

somewhat surprising that recent discussions of Soviet R&D style,* including that aspect, do not remark that one who is required to honor the dogma that doctrine drive technology, but observes that it does not, will ordinarily prefer conservative technology in responding to requirements. Even the most doctrinaire Russian R&D specialists must have noticed that *demanding* some technical achievement does not have the desired effect unless the requisite technology is ready at hand. Clever re-integration of proven technology, frequent small advances, and demonstration of system capability *before* commitment to operational service characterized Soviet aircraft and tracked-vehicle development from the 1930s at least into the 1960s. Recent Russian departures from those patterns have had unhappy consequences. The exceptionally long delay in availability of the Soviet Il.86 wide-body civil transport and the complete failure of the Tu.144 supersonic transport program are cases in point.† Because all Russian aerospace programs can be treated as extensions of military R&D enterprise, it is likely that similar failures have occurred in similar military programs. Soviet military security would have prevented their coming to public notice.

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*See, for instance, Col. R. G. Head, "Technology and the Military Balance," *Foreign Affairs*, April 1978; and A. J. Alexander, *Decision-Making in Soviet Weapons Procurement*, Adelphi Paper No. 147, International Institute for Strategic Studies, London, 1978.

†It is an interesting commentary on the quality of three different R&D styles that the United States abandoned its infant SST program as soon as a government subsidy of costly, high risk technology was withdrawn; that Britain and France elected to proceed with economically calamitous compromises of technology in the prestigious but costly Concorde program; and that the Russians incurred economic and technological catastrophe as a consequence of ignoring both the American precept and the Russian tradition of technological conservatism.

For at least the past decade, the Soviets have been credited with having extensively exploited the traditional advantages of incremental development. A leading spokesman for that view has been Arthur Alexander, who has pointed out the benefits of incrementalism, cost consciousness, and design conservatism--which the United States often forgoes in enthusiasm for new technology.[*] Dr. Alexander Flax has argued that such generalizations oversimplify several important issues. He takes the position[†] that Soviet R&D authorities have been as technologically ambitious as their American counterparts and that Americans consistently and effectively exploit incrementalism. David Holloway[‡] has observed that Soviet designers and R&D managers, even at the highest military and political levels, apparently *prefer* to invest in design improvement and incrementalism *if* that is possible, but in instances when doctrinal goals cannot be so satisfied, "Manhattan Project" enterprise styles may be adopted. In such instances, Soviet technology has been hard pressed to maintain the pace required of it.

This would be heady stuff for graduate school seminars if it were not so vital a determinant of the comparative military readiness of the two countries. Is Soviet style no more than incrementalism leavened by random breakthrough efforts? Is it the other face of the American coin? Does it matter?

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[*] *Decision-Making in Soviet Weapons Procurement.*

[†] *Foreign Affairs,* September 1978, pp. 207-211.

[‡] "The Soviet Style of Military R&D," paper prepared for the Cornell-Rockefeller Workshop, March 1979.

One approach to such questions is to begin with the assumption that there is a nationally preferred Soviet R&D style, although (as with Soviet military doctrine) expediency, pragmatism, and "special circumstances" may condition Soviet responses to particular challenges. Further, it seems obvious that in a great many ways the customary Soviet R&D approach in (for instance) tank development is different from the way the Soviets developed their earliest ballistic missiles. But it is also conceivable that the present Soviet preference for product improvement in ballistic missiles may be no more than a new expression of the Soviet tradition of incremental development.

Institutional inertia makes it unlikely that one approach can quickly be substituted for another in Soviet military R&D; thus a rapid shift from incrementalism to concurrency would be difficult. The conventional, safest, most acceptable, most familiar, and therefore the institutionally preferred Soviet approach is incrementalism.* But on occasion circumstances compel the Soviets to attempt to ingest large quantities of risky technology to match or offset new systems and concepts introduced by the Americans. (Sub-launched ICBMs, MIRVs, and precision-guidance cruise missiles are examples.) Dedicated and expensive efforts to advance technology on a schedule endorsed by the Kremlin have been unsuccessful in various respects

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*Surprisingly, political scientists have not remarked on the chasm between Marxist R&D doctrine--innovative, unchained Communist technology will overwhelm capitalism--and recent Soviet R&D practice--conservative gradualism with an admixture of irrational adventurism.

(consider the SS-10 missile and the high-bypass turbofan engine, for example), partly because technology is no more respectful of commands sternly voiced in Russian than in English, but mostly because Soviet dogma, bureaucracy, and institutional and cultural rigidities impede Soviet efforts to perform the rapid programatic and funding starts, stops, and changes of direction that characterize--and are essential to the success of--the high risk, high technology aspects of U.S. military R&D.*

The best of American military R&D is characterized by pragmatism, adaptiveness, flexibility of approach, and a decent respect for the occasional intractability of technology. The striking advances often made by relatively small groups led by skillful, imaginative American innovators attract much attention. But it is also true that much vital American military R&D depends on recurrent increments of performance improvement that are increasingly difficult to achieve. Who is to say that two decades of continuing small advances in the

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*Robert Perry, "Verifying SALT in the 1980s" (in *The Future of Arms Control: Part 1, Beyond SALT II*, Adelphi Paper No. 141, C. Bertram, Editor, International Institute for Strategic Studies, London, 1978), suggests that such institutional factors prevented the Soviets from successfully competing with the Americans in certain categories of strategic weapons development and that the Soviets probably would demand a cessation of American R&D in some areas as a price for agreeing to reduction in strategic weapons inventories. Alternatively, the Soviets could extend and intensify their known efforts to adapt and exploit "American R&D management techniques" that would improve their ability to compete in areas of high risk technology. Although the perplexing and self-contradictory nature of Soviet R&D has been the occasion for an enormous literature, a void remains: the policies, quality, and output of Soviet R&D deserve at least as much attention as has been devoted to the organization, infrastructure of, or the resources consumed by, that institution.

technology of the Sidewinder missile are of less military value than the spectacularly rapid creation of the F-16?

It seems safe to assume a present American advantage in some potentially critical areas of R&D enterprise, but an advantage that arises more in process and practice than in some broader mastery of the philosophy of R&D. Whether such an advantage can be extended or preserved remains uncertain. Russians have become keen students of the U.S. R&D management techniques to which the American advantage is often attributed. But in their mix of R&D approaches, the Soviets still seem to be consistently less successful than the Americans. If Russian systems are cheaper, they also tend to be less advanced,* or at least, advanced in ways that the United States does not value as highly. And the Soviets characteristically are slower to respond to newly perceived technical needs.

In some respects the persistence of an American R&D advantage seems surprising: U.S. military R&D has malfunctioned in many ways for many years. Although American R&D managers typically respond to the identification of system-specific problems by generating uniquely appropriate solutions, they also generalize from those solutions and apply them indiscriminately to other systems with unrelated problems, thus creating new system-specific problems which then require new unique solutions, and so on.

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*Notwithstanding disagreements about the comparative virtues of Russian and U.S. tanks, Foxbats and SR-71s, and U.S. and Russian jet engines, it appears to be generally acknowledged that U.S. systems *both* cost more and perform "better"--but differently.

Rather than attempt to enumerate the problems it seems sufficient to list some of the solutions (Chart 1) that have been developed and applied, successfully or unsuccessfully, by some sector of the American military in the past 15 or 20 years.

INSTITUTIONAL	PROCEDURAL
Management Techniques	**Planning**
-"Red Line" (F-15)	-"Independent" Cost Estimates
-Milestone Reviews	-Life Cycle Costing
-Reorganizations	-Technical Risk Assessment
-Decisions at Ever Higher Levels	-Cost Performance Tradeoff Models
-Secretary of Defense	-Mission Element Need Statements
-Congress	
-The Presidency	**Contractual**
	-Incentives
Development Strategies	-Cost Plus Fee
-Concurrency (ICBM)	-Fixed Price
-Multi-Service Weapons (F-111)	-Total Package
-Fly-Before-Buy (A-10, UTTAS)	-Warranties
-Prototyping (F-16/F-17)	
-Design-to-Cost	**Procurement**
	-Profit Controls
Test and Validation Variants	-Rewritten Procurement Regulations
-OSD Director for Test and Evaluation	-"Set Asides"
-Service Test Centers	

Chart 1: Solutions attempted

We already know that the routine application of "proven" solutions to newly perceived problems more often complicates than resolves them. An example? The perception that concurrent development and production were advisable in the terminal stages of ICBM programs of the late 1950s prompted the view that routine compression and overlap of the

sequential phases of all major acquisition programs could save time and money.* Translated into policy directives, that assumption eventually led to "Total Package Procurement," which in application had mixed outcomes; to several tightly compacted system development programs with marginally useful products; and to the near abandonment of such frequently beneficial approaches as prototyping, extended testing, product improvement, and low-initial-rate production. That the inventiveness or energy of solution-contrivers may be flagging is implied by the recent revival of interest in concurrency and by renewed arguments that prototyping and proof testing are "too slow," "unnecessary," and "too costly."

Moving past the rubrics, the easy solutions, and the panaceas brings one to a fundamental question: of what is the R&D and acquisition process composed and how is it ordered? The following charts depict its principal elements as viewed from three different perspectives. Chart 2, showing the conventional relationships among the various phases,† reflects the assumption that each phase of acquisition is undertaken in sequence and is satisfactorily completed before another begins.

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*"Compared to what?" was treated as an unworthy question. When the assumption of cost savings proved unsupportable, the argument for concurrency became "the time saving justifies the higher cost." In the event, analysis failed to provide much support for the assumption of schedule advantages. See B. H. Klein, T. K. Glennan, Jr., and G. H. Shubert, *The Role of Prototypes in Development* (The Rand Corporation, RM-3467/1, April 1971).

†For purposes of comparison, the five phases and subphases of the Defense System Acquisition Review Council (DSARC) approval process are shown. Phases 3A and 3B are, respectively, approvals for long-lead-time and high-rate production.

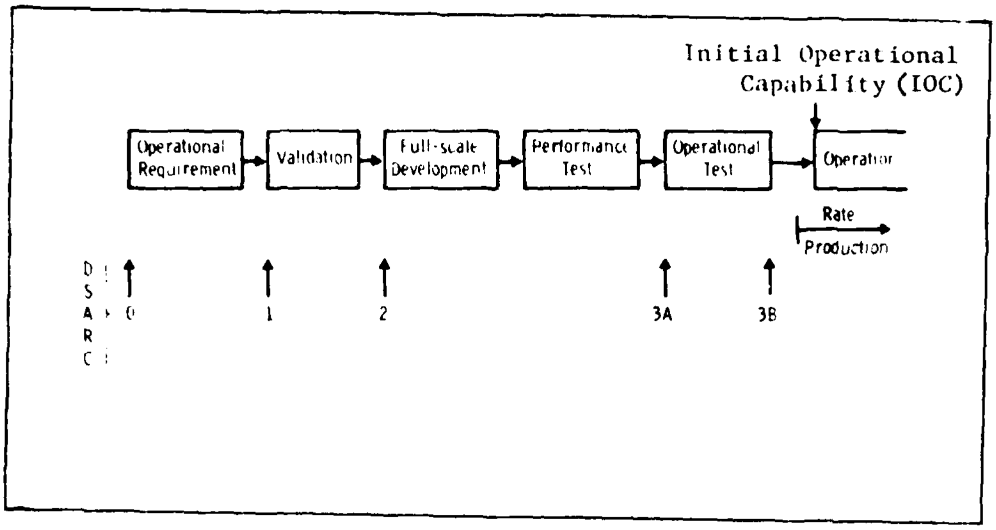

Chart 2: **Acquisition: the idealized conception**

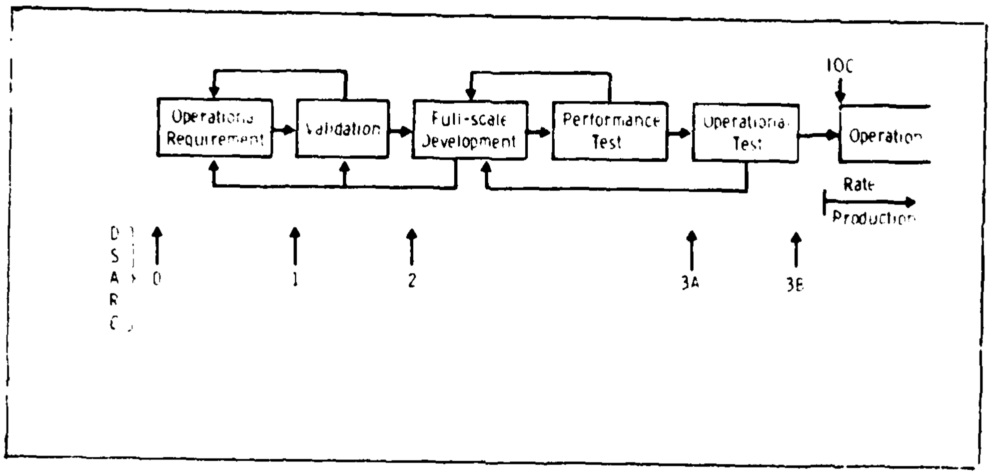

Chart 3: **Acquisition: the reality**

Chart 3 shows how development, test, validation, and production inevitably overlap and interact when program managers realistically address the main task of R&D: uncertainty reduction. Unless there is some compelling justification for risking cost overruns, schedule slippages, and system performance shortfalls, active feedback loops must

interconnect the various activities. Realistic programs feature recurrent iteration because the reduction of risk and the accumulation of relevant new knowledge compel frequent reassessments of the relationships between requirements and technology, between design and test, and between operational validation and redesign. Industries dependent on technological advance routinely operate in accordance with such principles. No acquisition program that includes research and development can proceed successfully by way of a simple sequential process. One particularly important relationship is that operational tests should be satisfactorily completed before a commitment to high rate production is made. There is, in the event, no other way of ensuring that standard production systems will perform as they are required to do after delivery to users.

The key to successful exploitation of the "realistic" mode of acquisition is the acknowledgement that *any* R&D program contains critical elements of risk and uncertainty. The insistence of program advocates that risk is minimal, or that all potential uncertainties have been anticipated and offset by procedural safeguards, is a routine predecessor of R&D difficulties.

Chart 4 represents the staging of acquisition functions in a fashion typical of American military R&D of the 1950s and 1960s. In many important programs, the interaction between operational requirements and technical validation was inadequate, encouraging a commitment to technically unrealistic goals. In the late phases of a program, an overlap of performance testing, operational testing, initial high-rate production, and actual service operations has been

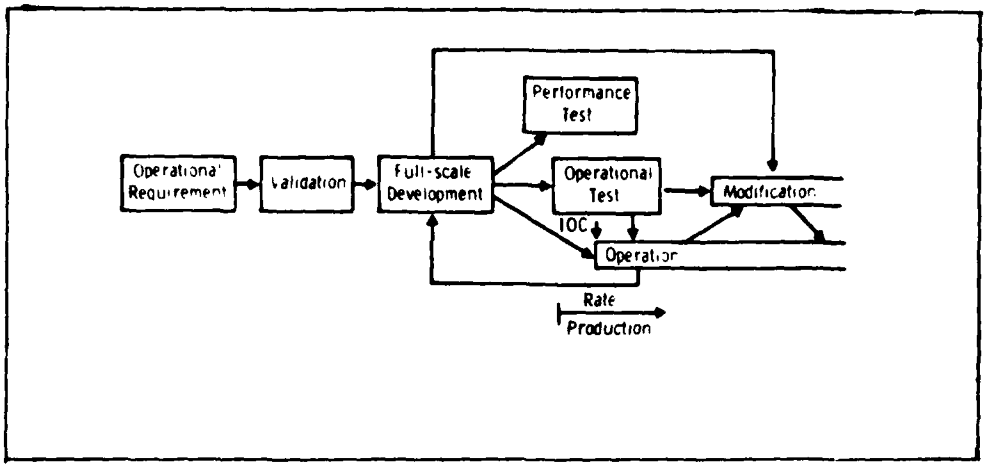

Chart 4: **Acquisition: the concurrency approach**

a classic response to schedule slippages that frequently originated in earlier technical difficulties. If test and production phases overlap to the extent that test findings cannot be applied to production articles, costly factory, field, or depot modification programs may become the only feasible means of correcting latent flaws in the delivered item. Such an approach often means that early production systems will not satisfy either the original expectations or the current needs of the users. The interval between the official Initial Operational Capability (IOC) date and the date of an effective operational capability may then be as much as two or three years. During that interval, systems may be hobbled by constraints on performance, or sidelined and awaiting modification. If the cost of advisable modifications exceeds the resources available, the expected operational capability may never be fully achieved. One classic example is the A-7D aircraft, planned for a sortie rate in

excess of three per day, but which when delivered proved capable of only one sortie per day. In effect, the Air Force either paid three times as much as it expected to pay for the A-7D's sortie capability or received only one-third of the capability it paid for.

Although program cancellation is--conceptually at least--an alternative to the production of a flawed system, political, technical, or military considerations may make it infeasible. In such cases, some compromise of performance becomes the only real option.

One classic means of reconciling inadequate technological capabilities with excessively ambitious requirements statements was illustrated by the F-111 (TFX) program. Only if highly optimistic expectations of technical progress had been realized could the 69,000-pound F-111 (designed in 1962) have provided the combination of supersonic dash and extended mission radius that its designers--and purchasers--expected. Mostly because of weight increases (to 82,000 pounds) during development, the aircraft finally produced lacked that capacity. But without those weight compromises, the F-111 would have had *no* useful operational capability. An F-111 of 69,000 pounds could be flown only about 100 miles at Mach 1.2--and at that point would have exhausted its fuel.

The C-5A experience provides an example of the consequences of *not* making reasonable compromises during development. Although the aircraft was to be a very modest advance in the state of the art, the C-5A program was eventually acknowledged to be a highly ambitious technological enterprise. A major goal of the original program was to deliver two main battle tanks to NATO on an unrefueled mission over 2800 nautical miles. The C-5A as delivered could perform that task.

But the C-5A program was tightly constrained by design specifications embedded in the contract, precluding most tradeoffs:* About 14,000 pounds of airframe structure had to be removed to reconcile takeoff distance, gross weight, and fixed empty-weight requirements. Most came from the wing. Subsequently the Air Force concluded that the main structure had been so greatly weakened in the process that service life expectancy had been unacceptably reduced. If present plans (1979) are carried out and a "new" wing is retrofitted to all C-5A aircraft, the deleted 14,000 pounds will be restored. The cost is likely to exceed $1.3 billion (1977 dollars), and unless performance compensations can somehow be provided the modified aircraft will be unable to perform the mission for which it was designed.† The product of a determined effort to satisfy unachievable technical goals on an accelerated schedule was the delivery of a gravely flawed system at a cost greatly in excess of estimates for what was once alleged to be an "off-the-shelf" design.

One contributor to the outcomes of such programs as the F-111 and C-5A appears to have been premature commitment to high-rate production. Typically in the 1960s, both the decision to begin

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*Including costs. Each C-5A cost more than twice the original estimate and in the end only half as many were built as the Air Force earlier said were needed.

†One obvious but expensive remedy would be to retrofit more powerful or more fuel-efficient engines, a measure Lockheed proposed before high-rate production began but which was then turned down because it was inconsistent with the costs and operational schedules on which the "total package" contract was predicated. The most likely response would be to rely on aerial tankers for all full-load missions of more than about 2200 nautical miles.

high-rate production and the actual start of production preceded the completion of either development or operational testing (performance validation), and frequently preceded the *start* of operational tests.* Such early commitment to high-rate production interferes with or even precludes the effective feedback of test findings into the design (and redesign) of the production article. In such instances, the performance of the delivered article may be appreciably inferior to that sought when the program was approved. A common consequence is the costly and extensive modification of the delivered system or the delivery of an incapable or marginally capable system, or both.

Although schedule compression and some degree of concurrency typified many DoD acquisition programs of the 1960s, that was not always the case. Perhaps the most obvious example of a shift from traditional incremental development and product improvement strategies to a "more modern" concept has been in the acquisition of tanks for the U.S. Army.

Since its beginnings in the 1920s, Soviet tank development has relied on the continual improvement of some existing, well tested armored vehicle. Until the early 1960s, U.S. tank development was also conventionally reliant on a product improvement strategy. Then, in a belated conversion to the "total system" concept originated by the Air Force, Army R&D philosophy changed (see Chart 5). Thereafter, the goal of Army R&D was to design mostly new tanks from the ground

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*The C-5A, F-111, A-10, and A-7D are examples.

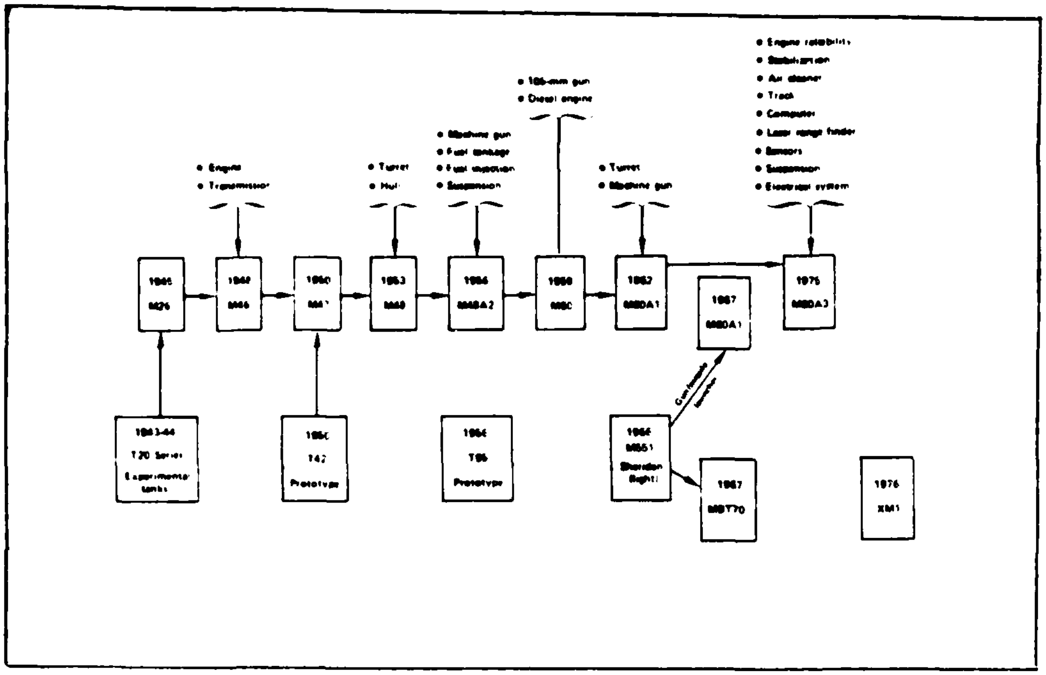

Chart 5: **How U.S. heavy tanks were developed**

up.* It is perhaps fortunate that traditions of product improvement could not be instantaneously dispensed with because in more than ten years of trying the "total system" concept did not bring on an "all new" production tank. Indeed--and in the event, fortunately--a further improved version of the M-60 (of 1959 vintage) was entered into production after the "new" tank concept was adopted.

One potential explanation for the difficulty of developing a "total system" tank may be that breakthrough technology is not an applicable option in tank development. Improvements come so slowly there and radical advances so infrequently that only derivative

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*See Alexander, *Armor Development in the Soviet Union and the United States*, The Rand Corporation, R-1860, September 1976.

designs are compatible with cost-effective progress.* Another difficulty is that institutions accustomed to incrementalism cannot quickly accommodate to new ways of doing things--a constraint that applies to 50-year-old Soviet tank design institutions as much as to their American equivalents.

Major institutional pressures on the U.S. military R&D process have been generated by assumptions about what endangers an approved program. A common argument against incrementalism as an R&D strategy is that it delays receipt of production approval. It is widely believed in the military that new equipment designs constitute a more effective hedge against an uncertain technological future than "improved" versions of older systems or subsystems. And tests often are directed at establishing *how* components will perform rather than *how well*, or *how long*, or whether an integration of newly developed components will function effectively at all. Proponents of breakthrough-style R&D maintain that it is not necessary to address such questions before production approvals have been granted, and that attempting to answer them all before scheduling a production start can delay or even prevent progress toward operational availability.

For what then seemed to be good reasons, an R&D strategy featuring compressed schedules, overlapping phases, and "total system" concepts was generally adopted by the Air Force in the 1950s and was either adopted by or imposed on the other services in the 1960s. The

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*Although technical obstacles have been common, the chief impediment to development of an "all new" tank has been its potential production cost.

usual program outcomes included cost growth, schedule slips, and performance shortfalls. Some of the remedies--the "solutions" earlier alluded to--were contrived to mitigate the consequences of performance and schedule shortcomings. Frequently, they worsened the situation. Very real concern about the state of defense R&D was voiced by the late 1960s. In itself, that was nothing new or unusual, but a greater pragmatism became apparent in changes that were proposed thereafter. Whatever their inspiration may have been, R&D policy changes adopted in the early 1970s came to be known as the Packard initiatives, for Deputy Secretary of Defense David Packard, one of the more outspoken advocates of fundamental reform in the defense acquisition process.

What were these changes, and what was their effect? For answers we can turn to a recent examination of 32 major systems* that entered full-scale development during the 1970s. Together, the systems in the sample accounted for more than $100 billion of DoD investment in research, development, production, and initial support.

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*The list includes:

Army		Navy		Air Force	
UH-60	HELLFIRE	AEGIS	LAMPS	F-15	ALCM/GLCM
M-198	AH-64	CAPTOR	SURTASS	B-1	
MICV/IFV	XM-1	AIM-7F	F-18	AWACS	
PATRIOT	DIVAD GUN	AIM-9L	TACTASS	A-10	
ROLAND		HARPOON	TOMAHAWK	F-16	
COPPERHEAD		CONDOR	5", 8" PROJECTILES	DSCS III	

Because the research was concerned with production systems, the many large shipbuilding programs of the Navy were deleted from the list. Data were taken from Selected Acquisition Reports--SARS--periodically prepared for submission to the Congress.

A major policy initiative to emerge from experience of the 1960s was emphasis on tests of actual hardware in lieu of theoretical analyses and "paper studies:" hardware demonstrations became increasingly prevalent after 1969. Indeed, 80 percent of the programs in the sample and all major programs that started after 1973 included some hardware testing before the start of full-scale development. To the extent that the data can be assessed, the availability of hardware test results seems to have been a considerable factor in approving or disapproving the start of full-scale development. Chart 6 shows a pronounced trend toward the increased generation of test data as the decade wore on.*

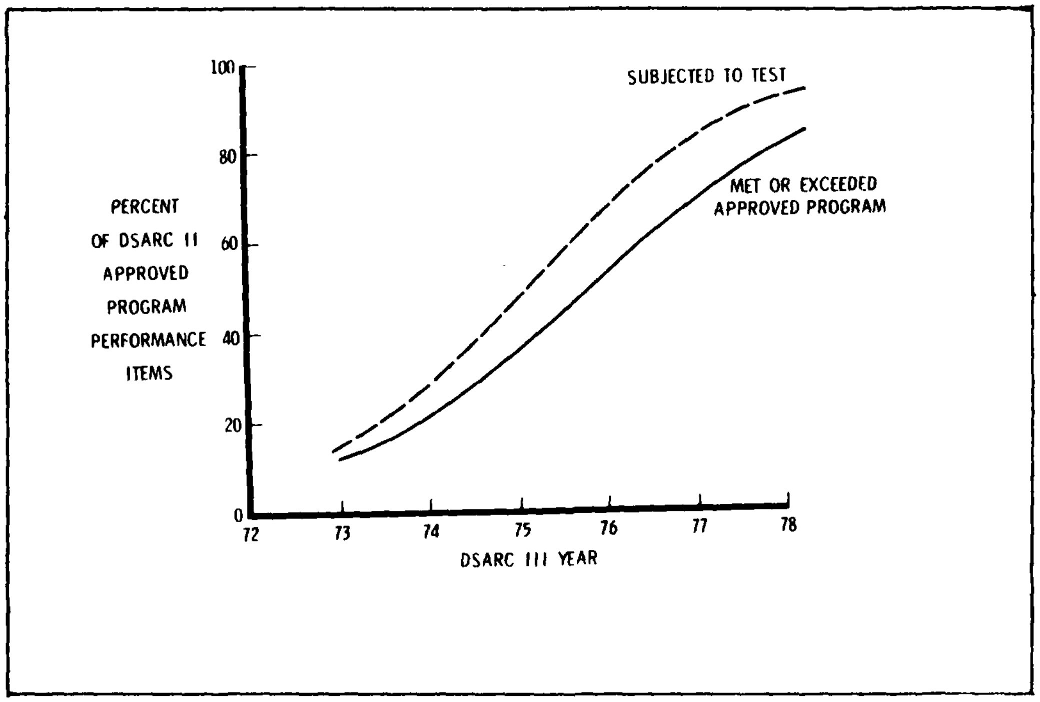

Chart 6: **Trends in performance testing**

*That the data actually were *used* cannot be proved. But they were available.

Greater reliance on hardware *competition* in selecting systems for full-scale development and in deciding when they were ready for production was another major initiative of the early 1970s. Although virtually unknown during the 1960s and only gradually effective in the early 1970s, hardware competition occurred in the predevelopment phases of two-thirds of the major systems that entered acquisition after 1973. Between 1969 and 1974, only 30 percent of the new acquisition programs included hardware competition during the years before full scale development began.

In three important categories, then, the process changes sponsored by David Packard actually took effect. Hardware demonstration became a regular feature of system development of the 1970s, performance testing showed a pronounced increase, and hardware competition became much more prevalent after 1973. Although the sample is small and the data are not extensive, the trends seem clear enough.

Were the effects of such changes beneficial, and if so, in what respects? Did they impose important new costs on the acquisition process? Did they lower performance standards, delay completion of programs, or make products less cost effective?

To determine the consequences of conducting acquisitions under the terms of the new practices requires examining the extent to which program cost, schedule, and system performance outcomes departed from the goals and objectives ("predicted program outcomes")

accepted by the sponsoring service *and* the developer at the time full-scale development began.*

Consider hardware competition. Where it occurred, what was its value? One way of evaluating effects is to compare actual with projected goals for programs that involved significant hardware competition during development and for programs that did not. The comparison must extend to total acquisition cost, development schedules, and system performance.

The results? The four programs in the sample that included hardware competition (AWACS, A-10, F-16, and UH-60) incurred substantially less cost growth and significantly fewer schedule slips than programs that did not involve substantial hardware competition (F-15, Aegis, Harpoon, AIM-9L, Captor and M-198). (See Chart 7.) System performance differences were not significant. Such indicators should be treated cautiously because of the small size of the sample, but the implications are provocative: where hardware competition occurred, it seemed to pay dividends.

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*The underlying assumption here is, of course, that the program advocates who in securing program approvals also set program goals consistently *understate* probable costs, *overstate* achievable performance, and *underestimate* development time. Two important definitions: First, the "predicted program outcome" is that given for the "approved program" in the first Selected Acquisition Report published after full scale development began. The baseline used for comparisons ignores subsequent adjustments to those approved program projections. Second, the "actual" cost used in these calculations is a late 1977 cost adjusted for inflation and for any quantity changes. The performance and schedule outcomes used for baseline comparisons are those actually reported to OSD by the services.

	ACTUAL OUTCOME/APPROVED PROGRAM	
	COMPETITIVE	NON-COMPETITIVE
TOTAL ACQUISITION COST	1.16	1.53
DEVELOPMENT SCHEDULE (MONTHS FROM DSARC II TO MILESTONE)	1.08	1.22
SYSTEM PERFORMANCE	1.03	1.00

Chart 7: Some effects of hardware competition

A different but equally encouraging effect of hardware competition was observed in several programs in which full-scale system prototypes were built and tested before or during full-scale development. In three instances (A-X, the Lightweight Fighter, and the Advanced Attack Helicopter), test program participants were convinced that the design that won the prototype hardware flyoff competitions would not have been selected had only paper designs been evaluated. Although not quantifiable, the effect of building competing prototype systems before full-scale development was to select a "better" system. In any case, hardware testing--prototyping--seems to be beneficial.

Without considering (for the moment) what the causes or trends may be in the 32 programs here sampled, cost growth (the excess of incurred costs over planned costs, in constant dollars) averaged 20 percent, exceeded 100 percent in one instance, and was negative in three instances. Over the first eight years after the start of full-scale development, cost growth averaged 5.6 percent per year.

Larger--more expensive--programs incurred proportionately less cost growth than small programs. One obvious explanation for the continuing cost growth trend lies in the unpredictability of R&D: older programs incur more cost growth than newer ones because they are exposed to larger numbers of unpredicted events over time.

Which prompts the question of whether cost growth is more pronounced during development or during production. Conventional wisdom holds the former to be more common, but most of the seven production programs in the sample exhibited growth during both phases (see Chart 8). The most obvious exception, the UH-60 helicopter, was also the only program in the entire sample in which direct competition between two contractors was maintained throughout full-scale development. That is particularly interesting, given the earlier indications of the benefits of competition, but not too much should be made of a sample of one.

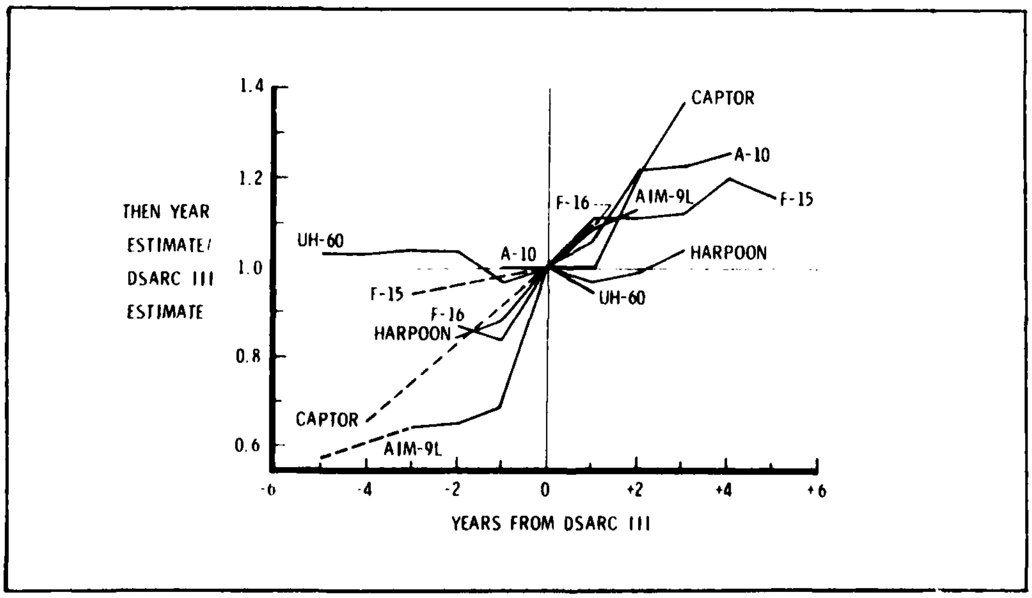

Chart 8: Program cost trends in development and production

Since 1969, program schedules have been about 13 percent longer than anticipated, although in a few instances program milestone achievements took twice as long as had been planned. System performance departures from initial plan were few: the average system in the 32 programs in the sample achieved its performance goals, but individual variances ranged from half as "good" to twice as "good" as expected.*

As compared to some recent civil projects (like the Alaskan pipeline, which cost about five times as much and took twice as long as promised), the defense programs in the sample appear to have done rather well. Further, all of the comparisons made thus far suggest that the policy changes instituted in the early 1970s, prompted by several studies of flawed policies of the 1960s, had beneficial effects in particular areas.

But were *overall* program outcomes of the 1970s "better" than those of the 1960s? Can one say with confidence that prototyping, increased testing, more careful program review, and greater program competition paid off in terms in schedule, cost, and system performance?

A baseline for such comparisons exists. In 1969, a Rand study team compared the actual to the predicted outcomes of 24 DoD acquisition programs of the 1960s. Although smaller than the more recent sample and containing a different mix of systems, it nonetheless is comparable. Techniques that permit researchers to correct for

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*"System performance," as measured here, is actually a composite of many performance features called out in the individual program specifications.

differences in the technical difficulty of programs in the two decades do not exist, so inter-decade comparisons of schedule and cost growth trends may be mildly suspect. Moreover, changes in ratios of actual to predicted outcomes may derive from shifts in the value of the numerator *or* the denominator, or both. That is, a perceived reduction in typical cost growth could be the product of better cost control or better estimating or some combination of the two. Small differences could signify little.

But average cost growth in programs of the 1970s has been only half of that experienced by systems of the 1960s (20 percent rather than 40 percent).* Relatively smaller differences existed in scope of schedule slip and system performance.† All differences between the 1960s and the 1970s show the later decade to have been "better" (see Chart 9).

Because the sample from the 1960s contained no development programs less than three years old, similar "young" programs were deleted from the 1970s sample. That adjustment raised the average cost growth increase for the 1970s to a factor of 1.34, not much better than the 1.40 of the 1960s. But when the ratio of actual to anticipated values

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*In fact, the typical cost growth of the programs of the 1960s in the Rand sample was ultimately *greater* than 40 percent because some of the more costly programs experienced their greatest cost increases after the data sample had been evaluated. A similar consideration may affect the 1970s sample, although no evidence of it was available by 1979 and the typical annual *rate* of cost growth was appreciably lower in the 1970s.

†But for the prototyped systems of the 1970s (there were no prototypes in the 1960s sample), performance outcomes were significantly better than for systems of the 1960s, and schedule outcomes were modestly better.

	ACTUAL OUTCOME/APPROVED PROGRAM (AVERAGE OF COMPLETE SAMPLES)	
	1960s	1970s
Total Acquisition Cost	1.40	1.20
With "Young" Programs Deleted	1.40	1.34
Weighted For Program Size	1.47	1.20
Development Schedule (Months from DSARC II to Milestone)	1.15	1.13
System Performance	1.05	1.00
Annual Cost Growth Rate (above inflation)	7.4%	5.6%

Chart 9: Inter-decade comparisons

is recalculated in terms of the total *costs* for all programs in the sample (thus weighting for program size), the "mature" 1970s programs sample shows a growth ratio of 1.20, less than half that of the 1960s (1.47). That difference is impressive. Finally, when the two samples are adjusted to equalize program age, the 1970s programs show real annual cost growth rate 25-percent *lower* than the 1960s. (That is, 5.6 percent in the 1970s against 7.4 percent in the 1960s.) Thus there can be no doubt that, since 1970, significant improvements have occurred in the predictability *or* the control of program costs, *or both*. Owing to peculiarities in the data, it is not possible to demonstrate that the improvement is more nearly 60 percent (the apparent upper bound) than 30 percent. But at the least, the net constant-dollar value of

the improvement is more than $10 billion for the 32 systems in the sample. However that may be accounted or apportioned, it is no insignificant sum.

Has that achievement cost something not captured in the calculation? One issue prominent in discussions of acquisition policy in 1979 is the length of time today needed to complete a program. The difficulty of doing much with the question arises in defining, and then determining for each of many systems of recent decades, when development began and when it ended. The excursion attempted here considers aircraft programs of the past 30 years.

Two caveats: First, the sample contains only aircraft systems. A comparably adequate set of data covering missiles or armored vehicles or other significant pieces of military hardware apparently does not exist.[*] Second, the survey concerns only that portion of the acquisition cycle that occurred *after* the beginning of full-scale development (as currently defined).[†] Although some other studies have concluded that acquisition programs lengthened in the 1970s because of delays in decisions during planning and concept formulation phases, the data available for this comparison would not support findings on that score.

For the evaluation, the acquisition time histories of 37 aircraft were measured from the start of full-scale development to first flight of the first airplane model produced under the development contract

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[*] At least there is no record of it at Rand.

[†] Pre-1970 aircraft milestone data were adjusted to be consistent with the definition adopted for DSARC II: the start of a formal, funded, full-scale development effort.

and to delivery of the first operational item to an operational squadron.

Three recent prototype programs (A-10, F-16, F-18) complicate the assessment of program initiation. By one mode of measurement the start of prototype development marks program start, although formal full-scale development did not actually start until DSARC Milestone II, which followed prototype demonstration and final source selection.*

Using the early start dates for the three prototype programs produces results which show that both time to first flight and total development time (reflected in time to first delivery of an operational aircraft) increased slightly over the past 30 years. If DSARC II dates are taken to indicate program start points for the three recent prototype programs the results suggest that average total development time *decreased* modestly during the three decades and that time to first flight remained constant.

Extending the time measurement to include delivery of the first 200 operational aircraft is also complicated by alternative interpretations of the start dates for the recent prototype aircraft programs. The pessimistic interpretation of the data shows that total acquisition times (including a substantial production phase) have increased by about 25 percent for aircraft systems in the past 35 years. But

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*In some programs of the 1950s, prototypes were built as an initial step in the development process. For such cases, the start of prototype work was taken to represent the start of full-scale development. Although conservative, that premise seems realistic--for the 1950s. In all cases, simple least-square trends were fitted to the data.

if DSARC II start dates are used for the two recent prototype programs,* a flat trend line results.

Some decrease in average production rates is also evident. That is not surprising, considering that current aircraft are as much as ten times more expensive (in terms of unit cost as a fraction of the total acquisition budget) and three to five times as heavy as comparable aircraft of the 1940s and early 1950s. In terms of constant dollars or pounds of aircraft per unit of time, however, production rates have not appreciably decreased during the past 30 years.

If, therefore, the total time needed to acquire new aircraft has been somewhat extended since the end of World War II, the change must be mostly credited to lower production rates. Other available data indicate that derivative aircraft take less time in development than entirely new designs, but that is not likely to surprise anyone. And, of course, the available data do not support any conclusions about trends in the time required to proceed from system concept (or some similar milestone) to approval for development. As yet, a data base and measurement methodology appropriate for such calculations are lacking.

Observations

If the American military R&D style is difficult to categorize or to define, it is at least possible to suggest that outcomes have improved in recent years, that the process works reasonably well, and

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*The F-18 was omitted because at the time the data were normalized no 200th production article had been scheduled.

that by all the standards we can apply it is increasingly effective. It is changing, in considerable part, because the Department of Defense has largely abandoned efforts to order up technology without regard for costs. Caution and risk aversion seem to be more pronounced both in selecting new systems and in committing to production. Consider that for various reasons, mostly quite sound, the B-1 and the American SST were cancelled. Somewhere in Russia the people who ordered the Tu.144 into production may envy the disorderly, unruly R&D style that led to such abortive developments.

American military R&D style is characteristically inconsistent in many aspects. It includes both derivative designs and innovative. For both, practicioners have (recently, at least) become rather adept at selecting and exploiting advanced technology suited to the needs of the time. Few major U.S. military development programs of recent vintage have required great inventive leaps into the unknown for success; on the contrary, the Department of Defense has increasingly invested in early hardware testing, and all the evidence says the benefits have been substantial. If postponement or cancellation of prominent but disappointing U.S. programs occurs less often today, it may be that fewer inappropriate programs progress far enough into development to invoke such Draconian measures. American R&D managers appear to have become better judges of the likelihood that a given rate of technological progress can be maintained. And they have become more adept at creating and preserving desirable technological options. This is not to say that all is well in the halls of military technology, or that further improvement is not needed. But the sky is not falling.

Whether Soviet or American, R&D institutions as readily aspire to organizational immortality as do trade guilds or cavalry regiments: instinctively, they resist change. In general, the USSR prefers an R&D process that proceeds from the orderly improvement of previously developed systems; and for the last 35 years the United States has preferred starting systems from scratch and seeking bold technical advances. Of course there are exceptions to both generalizations and, in the American case, styles are changing. Several political and institutional factors inhibit the Soviet creation of new R&D or production organizations and encourage the continuance of established institutions that favor an R&D style based more on derivatives than new initiatives. A tightly integrated planning structure, centralized resource allocation, and unwillingness to countenance unemployment tend to inhibit quick changes of process, product, or approach in Soviet military R&D. If the need is sufficiently great, the Soviets (with undisguisable difficulty) establish new enterprises; when the benefits of incrementalism appreciably diminish, they reluctantly invest in wholly new designs. The United States innovates more skillfully, more routinely, and with less anguish. The Soviets appear to lack the doctrinal and procedural flexibility that is characteristic of American military R&D. With all its impediments, the American system is astonishingly resilient, able to survive rapid starts and stops and changes of direction, scope, or goal. It accommodates effectively to technical, financial, and procedural "instability" notwithstanding the noisy complaints of some who would prefer a less uncertain future and a more monolithic institution.

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Skillful managers are able to create new initiatives and to compose alternative goals, to reallocate resources, and to exploit unheralded technology. Such opportunities are not automatically provided by the more doctrinaire R&D institutions of the Soviet Union, even if those units are more generously funded and more heavily staffed. In areas where technology is changing rapidly, where new initiatives are frequent, where both payoff and risk are potentially large, the U.S. military R&D style~~, whatever its shortcomings~~, has a decided advantage over that of the Soviet Union. At the end one is tempted to paraphrase a comment on democracy attributed to Churchill: our's is the worst possible way of doing R&D--except for all the others.